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The Appleton - Massachusetts Historical Society Rhode Island Ship Token with Vlugtende

by Michael Hodder

(TN-129)

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CNL Patron Dr. Phillip L. Mossman presents "Connecticut Revisited"

and Letters from our Patrons.

Were There Three (or More) Rhode Island Ship Tokens With *Vlugtende* on the Obverse?

by
Michael Hodder
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(TN-129)

Part I

Several years ago Carl Carlson mentioned to me that he believed that William Sumner Appleton had once owned a 1779 Rhode Island Ship Token *vlugtende* on the obverse. Carl told me that no one had seen that specimen recently, and its present existence was, therefore, unverified. I filed this intelligence away, for future reference.

During the winter of 1987 I mentioned the possibility that Appleton may have once owned a *vlugtende* obverse token to Miss Anne Bentley of the Massachusetts Historical Society, where Appleton had bequeathed his coin collection in 1905. Anne was intrigued, and promised to look though the trays of Appleton material that still remained at the Society. Shortly afterwards, Anne called to say that she had located the token, and that it did, indeed, have *vlugtende* on the obverse.

Needless to say, I was very excited at this news, and at the first opportunity I drove to Boston to see the token. My excitement soon turned into puzzlement. The token at the Massachusetts Historical Society did not look like any other I had seen. It was uniformly dark brown in color and lacked the edge scoring marks that nearly every other example shows. The design details on the high points were very soft, suggesting extensive wear, yet the surfaces appeared smooth and showed no signs of circulation. Perhaps it had been a pocket piece, kept as a souvenir for many years. This would explain the preferential wear patterns, but could not answer the questions raised in my mind by the lack of edge marks and the curious color.

The Garrett-Roper-Adams specimen, which has been catalogued both as "unique" and "one of two known", is owned by Mr. John Adams of Boston, who is a consultant to the Massachusetts Historical Society on numismatic matters. Through the courtesy of both Mr. Adams and the Society, the Garrett-Roper-Adams example and the piece from the MHS were sent to the American Numismatic Association Certification Service, for non-destructive metallic analysis by the x-ray fluorescence process. This test identifies the elements making up the surface of a coin or medal at the point on the surface where an electron beam impinges on the test object. The qualitative analysis is then repeated on several other points, yielding a quantitative reading which closely approximates the percentages of each element detected in the surface of the whole coin or medal.

X-ray fluorescence test results are valid for statements of the surface make-up of a test object. Under certain circumstances, however, they can be assumed to characterize the internal elemental makeup of a specimen, also. Although the electron beam only penetrates the surface of the test object to a depth of about 5 microns, if the surface composition is found to be physically stable, then the surface analysis can be taken as a guide to the internal composition of the object. This observation holds particularly for coins and medals composed primarily of copper and zinc, where no surface de-zincification is noted under magnification. X-ray fluorescence analysis of brass alloy coins and medals, then, can confidently suggest both the qualitative and quantitative internal composition of such specimens, always assuming that no microscopic signs of zinc depletion are noted during the analysis.

The results of the surface analysis of the two examples were as follows:

Appleton-MHS Specimen

Zinc:	46.03%
Copper:	53.08%
Iron:	.89%

Specific Gravity: 8.25
Weight: 153.9 gns.

Garrett-Roper-Adams Specimen

Zinc:	51.88%
Copper:	45.79%
Iron:	2.33%

Specific Gravity: 8.18
Weight: 151.1 gns.

Microscopic examination using polarizing light revealed no traces of surface de-zincification, and so it may be assumed that the quantitative analytical results of the surface composition reflect the internal elemental makeup of the two specimens, as well. The close similarity of composition between the two strongly suggests that they are composed of essentially the same alloy.

The photographs above are of the Appleton-MHS (upper) and Garrett- Roper-Adams (lower) specimens. Macro comparison of the two establishes conclusively that the former could not have been a cast taken from the latter. On the Appleton specimen, for example, the bottom stroke of L in ADMIRAALS is strong, while on the Garrett- Roper-Adams piece that letter is weak. Similarly, the E of DE is weak on the Appleton token, while it is strong on the other. Finally, the side of Howe's flagship is heavily worn on the Appleton token, while on the Garrett-Roper-Adams specimen full rows of gunports are visible.

Visual inspection, microscopic examination, and the results of the x-ray fluorescence analysis raised more questions in my mind than they answered. Although the Appleton specimen looked like a cast, had no edge scoring marks, and was of an unusual color, its elemental makeup showed it to be very similar to the Garrett-Roper- Adams token and it clearly could not have been a cast taken from that piece.

In the hopes of answering some of these questions, the Massachusetts Historical Society agreed to submit their specimen to Harvard University's Center for Conservation and Technical Studies for further analysis. Dr. Eugene Farrell of Harvard's C.C.T.S. was asked to confirm the results of the x-ray fluorescence testing done earlier, to inspect the surfaces under binocular polarizing microscopy, and to analyze the surface micro-structure of the token using the Laue back reflection x-ray diffraction technique. These further tests were completed by the early summer of 1988.

Dr. Farrell determined that the earlier x-ray fluorescence results for both specimens were accurate. He found that while there were no typical signs of surface de-zincification, the surface of the Appleton specimen appeared to him to have been copper plated! There was little copper on the edge of the Appleton token, so the high surface copper content seen could not have been the result of preferential copper segregation or extreme de-zincification over time. Here was another oddity about the Appleton specimen that had to be explained before its origins could be understood.

The x-ray diffraction tests showed the surface structure of the Appleton token to be very fine grained, ruling out the possibility that it was a 19th century cast. Dr. Farrell suggested in his report that it could only be either an electrotype copy or a cast made under conditions of rapid cooling ("chill cast"). He ruled out the former possibility, as he found no signs of a seam along the edge, further positing that electrotyping an alloy was a difficult process. He concluded that the Appleton specimen was a chill cast copy that had later been electroplated with copper.

To Dr. Farrell, the elemental makeup of the Appleton specimen was the single most compelling reason for believing that it could not have been made in the 18th century. His report stated "The high zinc content eliminates the coin from dating from the 18th century as indicated (1778-1779), since brass was made from the cementation process in the West until the 19th century. The maximum amount of zinc possible by that process was approximately 28% Zn by weight." Here was a real bombshell, for if the metallic content ruled out the possibility that the Appleton specimen was made in 1779, then the Garrett-Roper-Adams piece could not have been made then, either!

The common wisdom among metallurgists is that the direct production of metallic zinc by retort distillation was learned in the West in the 19th century from experiences with Indian methods, where zinc had been distilled for a century earlier. Prior to that date, European brasses were limited to a maximum 28% zinc content, due to the restrictions of the traditional calamine cementation process. But, what do we make of the Garrett-Roper-Adams token, which has always been believed to be genuine? Its elemental composition shows that the cementation limits had to have been broken, earlier than the common wisdom suggests was possible.

The answer to this problem came during a telephone conversation with Martha Goodway of the Smithsonian Institution's Conservation Analytical Laboratory. It appears that an Englishman, William Champion, had developed a vertical retort method for distilling metallic zinc in 1738, one which he patented in 1740. His works at Warmley, near Bristol (center of the British brass industry at the time) were producing metallic zinc before 1740. Therefore, the technology for making brasses with a very high zinc content existed before the putative date of striking the Rhode Island Ship Token, 1779. Further, Mrs. Goodway stated that chemical plating brass alloy objects with copper coatings was commonplace in the late 18th century. Therefore, the elemental composition of the Garrett-Roper- Adams specimen was within the technological capabilities of the late 18th century, and the only questions remaining were those that originally inspired this study about the Appleton specimen.

There is nothing in the elemental composition, surface structure (both micro and macro), or appearance of the Appleton specimen that violates the technological capabilities of the

late 18th century brass maker or coiner. The fine grain structure seen in the x-ray diffraction tests were not accompanied by surface dendritic structures, virtually ruling out the possibility that the Appleton specimen was made by chill casting. Fine grain surface structures result from three different methods of manufacture: electrotyping, chill casting, or hot striking between dies. We can rule out the former, as Harvard did, since no seam was found and the alloy mix would have been very difficult to electrotype deposit. In the absence of surface dendritic structures, chill casting is unlikely but not entirely impossible. Striking is yet to be proven.

Most numismatists realize that 19th century casting techniques applied to manufacture copies of coins were far more primitive than those used today to make counterfeits. Centrifugal casting, chill casting, and ceramic molds are modern processes as applied to making fakes or copies. Nineteenth century cast copies can usually be detected in x-ray diffraction test photographs by the irregular, large grain structures revealed by the diffracted x-rays. The Appleton specimen exhibited fine grain surface structures in the x-ray diffraction tests, ruling out the possibility that it was a 19th century cast copy.

Part II

At this stage in my study of the Appleton token it became important to know as much about the history of ownership of the specimen as possible, and I began making inquiries among numismatic bibliophiles, attempting to trace early auction appearances of Rhode Island tokens described as having *vlugtende* on their obverses. P. Scott Rubin, Carl Carlson, and John Adams were of great help at this point, cheerfully looking up references and sharing with me what they had found. I also read through my notes taken from the *American Journal of Numismatics* and *The Numismatist*, to see if there were any early references to the variety.

The story that emerged from this research has led to some surprising conclusions, ones which require a re-evaluation of the traditional rarity rating given to the variety. The Garrett cataloguer had called it "unique"; the Roper cataloguer suggested that two had once been known. My own study now suggests that at least three, and possibly four, different specimens had been known as recently as 1923. The whereabouts of the others were untraced now, but perhaps the newly found Appleton example was one of these.

In the spring of 1864 John King returned from a trip to Holland and brought 15 Rhode Island tokens to America. Before this date, the series was considered to be extremely rare, one example of the common type having been auctioned in 1864 for $40.00! Three of King's pieces were sold to a Mr. McCoy, while the balance went to Professor Charles E. Anthon. It is not known if any that King brought back with him was a *vlugtende* obverse variety, but it is very likely that one or more were included among the group. The first definite auction appearance of the variety I have found is in Woodward's 15th sale (December 1866), the Jencks-Paine Collections. Lot 1158 was described as a *vlugtende* obverse variety, and sold for $10.75. It was called "Good" in the catalogue, and was bought by George M. Parsons. Woodward's catalogue, of course, does not tell us whether this example was consigned by Jencks or Paine, but his own later description of the sale suggests that the piece had been Paine's. It was not a "stock" token, either, as Woodward said the sale had been "...for the account of the owners."

The next mention of the variety I found is in the *American Journal of Numismatics*, v. 2 (December 1867), p. 80. In that number, in a letter dated at Providence October 25, 1867, George T. Paine advised his readers "I have in my possession a complete 'set' (so to speak) in brass of the medal known as the "Rhode Island Medal". Two of the three I think I can trace to the Importation of Mr. King, which took place, I think, in the summer of 1864. The other has been in my possession a much longer time."

This is a curious communication for several reasons. Firstly, although Paine's statement tells us he must have owned a *vlugtende* obverse token, it does not tell us if it was one of the King imports. More importantly, Paine's advice is dated one year after the Woodward sale of portions of Paine's collection had taken place, and we know from the auction catalogue that a *vlugtende* obverse token was sold in that auction. If the December 1866 Woodward sale specimen had been Paine's, then in order for the variety to have been unique we must assume that he bought the piece from Parsons after the sale, since Parsons is recorded as the buyer of the lot. Otherwise, Paine either had purchased another specimen after the sale, or already owned a duplicate, making two known at the time. Alternately, if Jencks consigned the Woodward's 15th Sale specimen, then we would still have to assume that Paine bought it from Parsons if the variety were to have been unique then. Paine's letter to the *AJN* could not have been written <u>before</u> Woodward's December 1866 auction, and only published afterwards, since Paine had already exhibited the specimen he said he owned in October at the August 18, 1867 meeting of the Rhode Island Numismatic Association (cf *AJN* v. 2, n. 8, p. 78). By August 1867, therefore, the census of known specimens of the *vlugtende* obverse variety included at least one, and probably two, specimens.

William S. Appleton read Paine's letter in the December 1867 number, and replied to Professor Anthon (editor of the *AJN*) in the February 1868 issue, saying that he owned a different example of the *vlugtende* obverse token at that time. By this date, therefore, at least two different specimens were known, and if Paine had not bought back the one sold to Parsons in December 1866, or if Jencks were not the consignor to Woodward's sale, the census included three separate pieces.

In an obscure auction by Joseph Leonard & Co. (50 Bromfield Street, Boston), originally scheduled for December 8, then changed to December 22, 1870, lot 265 was described as a Rhode Island token with *vlugtende* on the obverse. It was called "Fine". We do not know who consigned this example, nor who bought it. Carl Carlson's copy of the auction catalogue has the pencilled notation "Mr. Crosby" on the outside front cover. The sale included U.S. coins and some remarkable early American issues, including Higley coppers. The cataloguing of the former was laconic, while that of the latter suggested to Carl that someone familiar with the series had written the descriptions. Whether Crosby catalogued those lots, or Carl's copy was Crosby's own, is, unfortunately, unknown. We may take it as certain, however, that the *vlugtende* token included was indeed the variety it was described as being, given the quality of the descriptions of the other lots.

Which piece was this? It could have been Paine's or Parsons', assuming one of them had sold or consigned his specimen. It could have been an entirely new example. It could not have been Appleton's, however, since his went to the M.H.S. 35 years later. If both Paine and Parsons still owned theirs in 1870, then the census would have to include four different examples. By the end of 1870, therefore, we know Appleton owned one and that there was at least one other, if we assume that Paine's, Parsons', and the Leonard sale

piece were one and the same specimen. However, this would mean that the second example was consigned by Paine to Woodward's 15th sale, bought there by Parsons, bought back by Paine after a few months, and then consigned to Leonard's sale three years later. Such a tangled pedigree track strains credulity. It appears more likely that besides Appleton's piece, at least two others existed by December 1870: one owned by Paine, another by Parsons. One of these may have been the Leonard sale piece. If not, then the census has to have included four.

Horatio R. Storer (Malcolm Storer's father) reported in the *AJN* in July, 1895 (vol. 30, no. 1, p. 27) that he owned a *vlugtende* obverse specimen, and that it was the only one known to him at the time. This was the piece C. Wyllys Betts knew and referred to in his catalogue. Nearly two years later, in June 1897, Storer exhibited his specimen at the monthly meeting of the Newport, Rhode Island Coin and Medal Club. At that meeting, Storer said that another example was then owned by a Mr. R. R. Barker of Newport. This makes two known to Storer in 1897, not counting the Appleton example which, eight years later, was bequeathed to the Massachusetts Historical Society. At this time, then, there were at least three distinct specimens of the *vlugtende* obverse variety in existence.

Where did Storer get his example? Possibly from Parsons, privately. Parsons died in 1895 (obituary in 10/1895 *AJN*), so the timing of Storer's ownership and Parsons' death is interesting. It is also possible that Storer had bought Paine's piece. Paine and Storer were both members of the Rhode Island Numismatic Association. What happened to the Leonard sale specimen? Did it find its way into Storer's collection, or was it the one Storer said Barker owned in 1895? Storer died in 1922, and his collection of coins and medals was divided between Boston Museum of Fine Arts and Harvard University. What happened to Storer's piece? Harvard does not own one today, nor does the BMFA have a record of one that they can find now.

Parsons' own collection was sold in two sales, by Ed. Frossard on October 16, 1865 and later by Henry Chapman on June 24, 1914. Neither sale contained a *vlugtende* obverse Rhode Island token. What happened to the specimen Parsons bought at Woodward's December 1866 sale? Did Paine buy it back from Parsons after the sale, thereby accounting for Paine's statement in the *AJN* in 1867? Did Parsons sell it privately to someone else? Was it the Leonard sale example?

Part III

We may never be able to unravel the tangled history of ownership of the *vlugtende* obverse Rhode Island Ship Tokens. Even Appleton's specimen is curiously silent in the records. The 1861 catalogue of his collection does not list the *vlugtende* obverse token, but we know from his letter to Anthon published in the *AJN* that he owned his example before February 1868. Appleton died in 1903, and his collection was bequeathed to the M.H.S. in 1905. There is, of course, no mention of Appleton's specimen in Crosby, since Crosby's work did not treat medals and the Rhode Island Ship token was classed as a medal when Crosby wrote. This is unfortunate, since Crosby relied on the Appleton Collection for much of his information on other series. Betts (561) did not mention a *vlugtende* obverse token in the Appleton Collection, either. When Walter Breen examined portions of the Massachusetts Historical Society's collection in the summer of 1952 he did not list tokens or medals. Therefore, apart from the brief mention of the piece in the 1905 inventory, the Appleton specimen has lain unnoticed in the M.H.S. trays until now.

But, is the piece at the M.H.S. genuine? Is it even the Appleton specimen? Over the years some rare early American coins bequeathed to the M.H.S. in 1905 have appeared in other collections, without explanation. In order to resolve these questions, we must return to the results of the scientific analyses mentioned earlier in this paper.

The x-ray diffraction tests showed that the M.H.S. specimen could not have been a 19th century cast, since its grain structure was atypical of casts made then. Further, Harvard's analyst found no signs of its having been an electrotype, either. If it were a cast copy, it would have had to have been made by chill-casting, a process unknown in 1905, and we would have to assume that it had been made after that date and substituted for the Appleton piece in the M.H.S. trays. Yet, no dendritic surface structures were observed under polarizing light microscopy, which is very atypical of chill cast copies.

The metallic analyses of the Appleton-M.H.S. and Garrett-Roper- Adams specimens, which seemed to Dr. Farrell of Harvard to tell against their authenticity, may actually support it. Prior to my own study of the variety, there had been no similar analyses done on Rhode Island Ship tokens, and there was, accordingly, no data available about their metallic content. It was not known that they appear to have been made from a brass which was unusually rich in zinc for the late 18th century. Published studies on the composition of brasses from this period always emphasized the limitations of the calamine cementation process, and the belief that 28% zinc content was the maximum obtainable had become gospel, so to speak. It seems to me that even a skillful counterfeiter wishing to manufacture a brass substitute token for the Appleton Collection piece could not have known about the high zinc content of the tokens, since that information has not been published before now. Therefore, a common counterfeiter would have used whatever brass was available to him; while a skillful one would have been sure to use one within the technological limitations of the late 18th century. An exceptionally clever counterfeiter, like the German Becker, might have used the metal from a genuine Rhode Island token, melted for the purpose.

The copper plating found by Dr. Farrell on the Appleton specimen is puzzling in the extreme. The high zinc content of the brass would have made freshly struck tokens exceptionally hard and brittle, but they would also glow with a color not unlike that of gold. Copper-plating one in 1779 would have defeated one of the purposes of the makeup of the melt. Why a later counterfeiter would wish to draw attention to his "creation" by copper plating it, thereby making an extreme oddity in the overall Rhode Island token series, is also difficult to understand. These questions will probably never be satisfactorily answered.

Is the M.H.S. specimen the same one that Appleton's will bequeathed to the Society in 1905? The metallic and structural analyses suggest that it was. Was it genuine even then? The results of the x-ray diffraction tests show it could not have been a 19th century cast, unless there is something about early casting techniques we still do not understand. Is it indisputably genuine? The answer must be NO, since there are features of it that cannot be adequately explained. Is the *vlugtende* obverse variety unique? Unless the Appleton specimen can be definitely proven genuine, or one of the two or three other specimens known by 1895 re-surfaces, the answer must be a qualified yes.

Late 18th Century Coinage Dies: The Metallurgical Processes Involved

by
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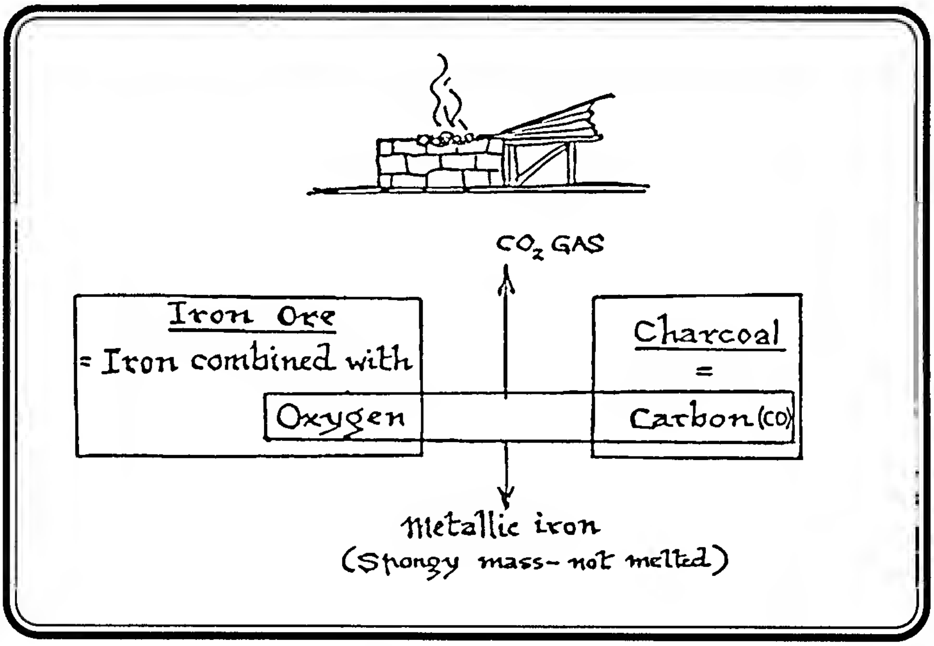
(TN-130)

We must first review how iron and steel were made in the 18th century. Iron, being a very reactive metal, is never found in nature as metallic iron. Iron ores are oxides - compounds of iron with oxygen. The rust which forms so readily on iron objects is an oxide. The most important ores are magnetite (Fe_3O_4), hematite (Fe_2O_3) and limonite (bog iron) ($Fe_2O_3 \times H_2O$). Limonite is formed by the deposition in streams and bogs of iron compounds leached out from subterranean oxide deposits. Bog iron was an important source of iron in the early history of iron-making but is no longer of significance.

The Bloomery



Illustrations by the author.

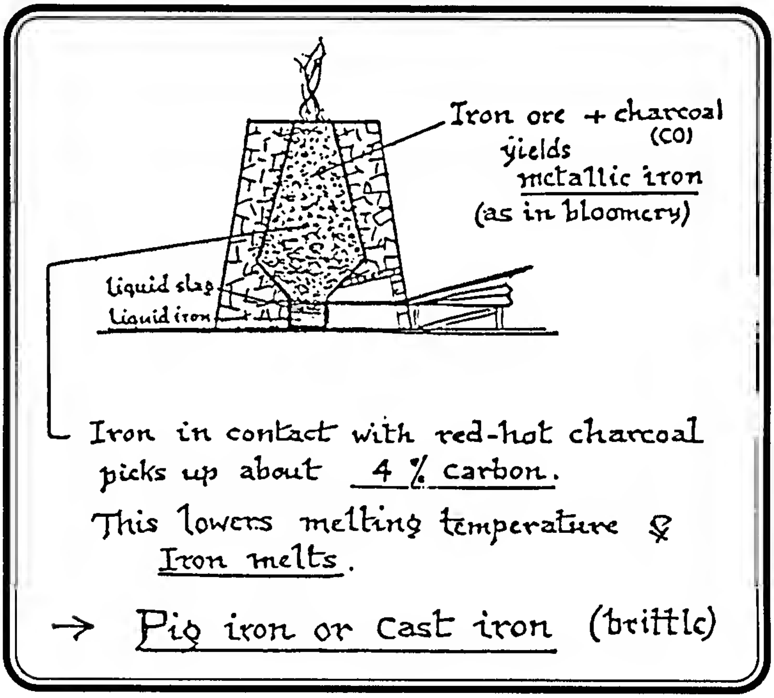
To make metallic iron it is necessary to remove the oxygen. The earliest ironmaking process, the bloomery, was a simple hearth, not very different to the traditional blacksmith's hearth, in which small pieces of ore were heated in a charcoal fire, urged on by a simple bellows. The charcoal was essentially carbon and this combined with the oxygen in the air on burning to produce carbon monoxide gas, which in turn combined with the oxygen in the ore to produce metallic iron and carbon dioxide gas. The temperature was not high enough to melt the iron, which was in the form of pasty particles which could be agglomerated into a lump of iron. This was removed from the fire and hammered to squeeze out the entrained charcoal and also "slag" - a dark brown glass-like product formed from reaction of the iron with the silica and other non-metallic compounds present in the ore. This slag contained a lot of iron oxide, so that the process was not very efficient. But it did produce metallic iron containing no significant dissolved carbon which could readily be shaped by hammering, hot or cold.

The bloomery produced only a few pounds of iron in a day but it was simple and required little capital investment. Surprisingly, this primitive process persisted in the less developed areas of the United States into the end of the nineteenth century. The *Directory to the Iron and Steel Works of the U.S.A.*, published in 1888 by the American Iron and Steel Association, lists a number of bloomeries, or "forges" as they were called at the time, and has the interesting comment:

> "In the mountainous districts of East Tennessee these forges are usually operated by farmers, who make iron from ore whenever it is needed in their immediate neighborhood."

The Blast Furnace

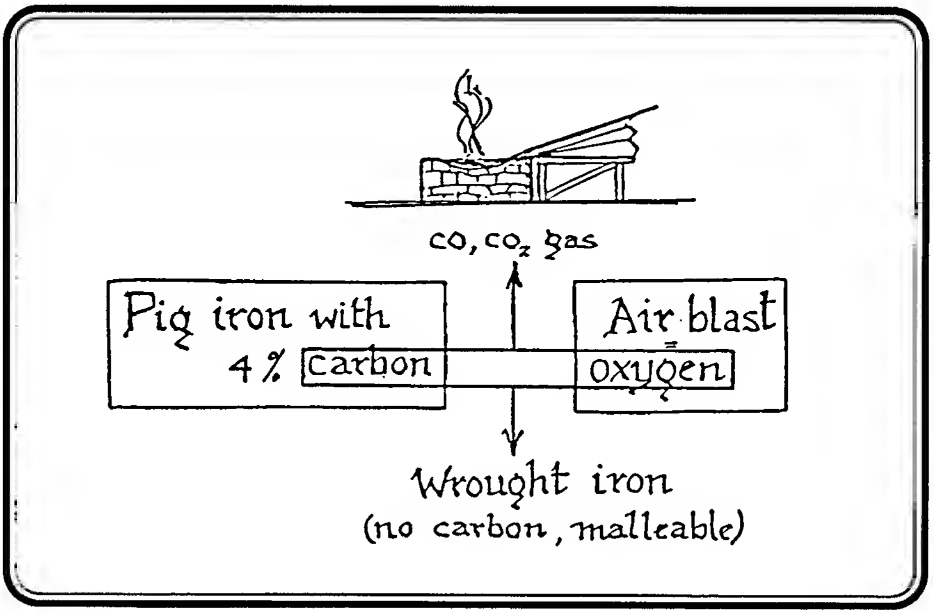
A major step in the technology of ironmaking was made in Europe in the fourteenth century with the invention of the blast furnace (or "high furnace"). This consisted of a stone tower about twenty feet high containing a vertical shaft lined with fire-resistant stones. Ore, charcoal, and a little lime as flux, were charged in at the top. The furnace was usually built against the side of a hill so that the charge could be brought across on a bridge to the top of the furnace. Air blast from a big water-powered bellows was introduced near the bottom. In the upper part of the furnace the same reactions took place as in the bloomery. But, as the particles of iron worked their way down through the hot charcoal they dissolved about 4% of carbon from the charcoal. Iron containing this amount of carbon has a much lower melting point than pure iron, so the particles melted and collected in the "crucible" at the bottom of the furnace as liquid iron. The slag melted and floated on top and could be run off to separate it from the iron. About every twelve hours the molten iron was run off into channels in the sand floor of the "casting house". This was "pig iron", so called because the side channels at right angles to the main runner were thought to resemble piglets nursing at the sow. The molten iron could also be run into shaped cavities (moulds) in the sand to form pots and pans, fire-backs, etc.



The blast furnace produced much more iron in a day than the bloomery, and gave clean separation from the slag. But there was a snag. Iron containing about 4% of carbon was brittle and could not be forged by hammering. The carbon was usually present as big flakes

of graphite running through the iron. But depending on the charge, the operating temperature and the amount of silicon present, the carbon could be present as iron carbide rather than graphite. This was "white Iron", so called from the appearance of the fracture. It was extremely hard but terribly brittle.

The Finery



To produce a malleable product from pig iron it was necessary to remove the 4% of carbon. This was done by remelting the pig Iron in a separate small furnace similar to the bloomery, and known as a "finery". The pig iron was melted in the charcoal fire and exposed to the air blast to "burn out" the unwanted carbon. As the carbon content fell the melting point rose, and the product of the finery was a mass of low carbon Iron which was removed and forged under a big water-powered hammer to produce a rough bar or "ancony". This was reheated in a separate hearth (a "chafery") for forging down under another hammer to a finished bar suitable for use by the blacksmith. This was "bar iron". It was extremely low in carbon content and was therefore malleable.

The Puddling Process

In 1784 in England Henry Cort invented the "puddling" process to remove the unwanted carbon. In this process the fire was in a separate compartment from the iron being processed, the flame from the fire being drawn over the iron by chimney draft. This made it possible to use pit coal instead of charcoal, an important advantage as charcoal was becoming scarce and expensive. The iron was melted and the carbon removed by reaction with the air, being stirred wlth long rods ("puddled"). The resulting low-carbon "ball" was removed to a hammer powered by a water wheel and forged into a useful bar. The original process was not very efficient as much of the iron was lost as slag. In 1830 Joseph Hall developed "wet puddling" (so-called from the amount of slag present). Hall incorporated iron oxide form old furnace bottoms into his hearth, which reacted with the high carbon iron to remove the carbon. It took about two hours to work down a 500 pound charge. Most of the wrought iron

used in the nineteenth century was puddled iron. It had a very low carbon content. Typically ironworks had batteries of many small puddling furnaces.

Steel

Low-carbon Iron cannot be hardened by quenching; It stays soft. And, of course, pig Iron and cast iron are not suitable for tools owing to their inherent brittleness. But if about 1 to $1^1/_4$% of carbon is present in iron the material can be hot forged to shape; and, if slowly cooled, is ductile and soft enough to be filed, cut, and engraved. It can then be hardened by reheating to red heat and quenching in water or oil. As quenched it is very hard but brittle. However, if "tempered" at a relatively low temperature in the range 400-600 °F, toughness can be progressively increased at a cost of some small loss of hardness. This versatile material is steel, used for knives, springs, tools and dies. The tempering (or "drawing") operation could be controlled by grinding a bright spot on the steel and watching the "temper colors" develop, from yellow at about 400° through purple to blue at about 570 °F. When the desired color was reached the part was cooled in water. The colors are "interference colors" caused by the progressive thickening of the oxide film on the steel as it is heated.

What is truly remarkable is that all this complex technology was empirical, handed down as jealously guarded secrets. It was not until the late 1800's that chemical analysis and the use of the microscope resulted in the development of a real understanding and valid theory regarding the role of carbon and what really happened during the processing of Iron and steel.

For generations and in many parts of the world steel was made from bar iron, which contained essentially no dissolved carbon, by heating it In a mixture of powdered carbonaceous material in a sealed refractory box for several days at red heat. Under these conditions the iron will absorb carbon, which diffuses in from the surface. Examination of the fractured surface of the bars showed a finer structure where the carbon had penetrated, compared with the coarser grain of the low carbon iron. When the fine grain was seen on the whole surface the iron was said to be "steeled through", which corresponded to a mean carbon content of about $1^1/_4$%. The gases produced resulted in a blistered surface; hence the name "blister steel". Careful forging of the steeled-through bars helped homogenize the structure. This method of making steel is called the "cementation process."

Huntsman Steel

A major improvement was made in 1740 by Benjamin Huntsman in England. He was a clockmaker and wanted to obtain a better and more reliable steel for springs. He developed a method of remelting the blister steel in crucibles to obtain a more uniform and homogeneous product. The blister steel always contained entrained inclusions of slag present in the bar iron used. The melting process allowed separation of this slag, giving a much cleaner and superior steel. (The slag particles tended to initiate cracking on quenching or in service.) Huntsman steel was already famous in Europe by the 1770's and may well have been imported by some knowledgeable American diemakers. (It was expensive.) It would have been a much better material for coinage dies than the unmelted blister steel.

Huntsman's remelting process was a great advance, but it introduced a new metallurgical problem. When molten metal solidifies in a mould, the first metal to solidify contains the least

impurities, which segregate into the last metal to solidify. Thus bar made from an ingot will vary somewhat in composition across the section and along the length. And, as the molten metal cools and solidifies, it contracts, leaving a shrinkage cavity ("pipe") at the top of the ingot. When the ingot was forged into bar it was important that the part containing the pipe should be discarded, but sometimes it was not all removed. Careful hot forging worked and refined the as-cast structure of the ingot to produce a bar with much improved mechanical properties - the more forging the betterl

The early Ironmasters had no chemical analysis of their ores or theoretical knowledge of the effect of impurities and trace elements present. They found empirically that some ore bodies produced better iron, and some produced iron that was "hot short" or "cold short", that is, was brittle either when hot forged or at room temperature. (We now know that these problems were caused primarily by sulfur and phosphorus respectively.) Swedish iron from the Dannemora district was famous from the 1600's for its superior quality. The ore was, as we now know, extremely low in sulfur and phosphorus and free from all but traces of other impurities. Huntsman, and other crucible steel makers, used Swedish iron, even although it commanded a premium price. Thus Huntsman steel was very low in impurities as well as being free of slag inclusions.

Making A Coinage Die

To make a coinage die the smith would have cut off a piece of bar steel of appropriate dimensions, either a piece of regular "blister steel" or, if available, from a forged bar of Huntsman "cast steel". The steel was heated to forging temperature (red heat), and forged to the approximate dimensions of the die. It was annealed by cooling very slowly in the ashes of a fire to produce a soft condition. It was then shaped in a lathe and by filing to the required dimensions. It was essential to remove enough metal to get below any surface seams and the decarburized layer below the oxide scale. The typical die was cylindrical in shape, with a short portion of the diameter of the finished coin rising from a base of somewhat greater diameter to fit the die holder in the press. The impression was engraved on the working face, an operation requiring great skill and artistic ability. The die was then heated in a fire to red heat for hardening, buried in charcoal to prevent oxidation of the impression. After being heated through at the right temperature the die was withdrawn rapidly and quenched in water. When cold it was placed on a hot plate, base down, with the impression oiled, and tempered until an area on the base which had been polished bright showed a straw yellow "temper color". (The impression end will have been slightly less hot as the heat penetrated up from the base.) The die was then cooled off in water. Any slight dimensional adjustment had to be by grinding the hard die. Finally, the impression was polished and the die was ready for use.

Die Defects

Coins showing evidence of die defects indicate one or more of the following problems: cracking; sinking of the center of the die due to inadequate hardness; and breaking out of irregular pieces of the die. The last type of defect may well have been caused by localized cracking initiated at included slag particles. There are a number of other metallurgical factors which could contribute to die problems. The carbon content may not have been optimum and undesirable levels of impurities may have been present. Forging can produce folds (laps) in the surface of the forged bar, which, if not completely removed by machining, could initiate cracking. If the forging temperature was too high, incipient melting of the grain boundaries

could occur, causing embrittlement. (Any attempt at forge welding would be liable to cause this problem). If the temperature employed for hardening was too high, grain coarsening could result, giving less good properties. The tempering may have been inadequate, leaving quenching stresses and an increased tendency to cracking in service. Whenever steel is heated in air it will oxidize and scale, resulting in loss of surface carbon content. To avoid this, the finished die will have been buried in the charcoal of the fire for heating for hardening, but any delay in air before the quench will have tended to cause decarburization and marring of the fine detail of the impression. It must be remembered that all operations from the making of the iron and steel to the final hardening and tempering depended on the empirical skills and judgement of experienced eyes. There was no chemical analysis to control composition, and no instrument to measure temperatures. (It is surprising, however, how well the trained eye can judge temperature from the color of heated steel from red through orange to white-hot.) It is not surprising that sometimes things went wrong; what is remarkable is that generally results were as good as they were.

The English Experience

Some very interesting and relevant correspondence has been preserved in the Birmingham City Libraries in England between Matthew Boulton, who minted great quantities of coins, and William Huntsman, son of Benjamin Huntsman who developed the Huntsman process. In April 1789 Boulton wrote:

> "I am about to undertake the striking of some millions of copper pieces which will require a hard blow in hardened steel dies. I have tried various kinds of steel but am not satisfied with any of them. I am of the opinion that the best steel you are capable of making will answer the best...It must be the best you can possibly make without any regard to price or expense, that being a trifling object in comparison to the quality of the steel...The steel I have hitherto tryed either cracks in the hardening or breaks afterwards in the striking or is so soft as to sink in the middle and become hollow, both which extremes I wish to avoid."

A month later Huntsman replied:

> "I have sent you twelve pairs of dies. The steel I send you will be sound and bear a great force being of good body."

In August he replied to another request:

> "I will send you ten pairs of dies to the pattern sent together with some steel for tools."

By next January, however, Boulton was dissatisfied, not apparently with the steel but with the forging of the dies:

> "I desire you will send me no more dies. But send me four cwt (hundredweight) of best steel such as you think will serve my purpose as I mean to have them forged under my own eye...You will know better than me which is the most proper."

The following May there is a further letter from Boulton:

> "I must beg of you to take the very best marks of Swedes iron to make the steel and that you will cast it into short thick bars suppose four inches square and then forge it down into bars about two and a quarter by one and a half which we will cut into proper size pieces. I prefer casting thick bars in order that it may take more forging for the more it is forged the better the steel...Please send one hundred weight as soon as possible."

It is clear that the eventual outcome was satisfactory since the trade continued well into the nineteenth century.

There is a most interesting letter from Matthew Boulton dated June 1797:

> "I believe I have discovered one cause why my dies do not stand as well as is necessary for I observe that such as are steel'd with some Bars that are one and a half inch square do not stand as well as those w(hi)ch are made out of the two and a half inch by one and a half inch, as the engraving of the first is made on the end of the steel whereas those Dies made from the two and a half inch steel the engraving is put on the Edge of the Steel and not on the end of it. In the one case the one and a half inch square is rounded by the hammer and then cut off in pieces about two and five eight long and a hoop of iron whielded (welded) round it. Whereas the large bars being two and a half broad a piece of one and a half is cut off it and then rounded by ye hammer and when forged into a die the edge of the bar becomes topmost. I have now not an ounce of your steel to work on and I must beg of you to send me as soon as possible some bars two and a half by one and a half of as good a quality of dies as you can make. Mr. Southerns (an agent) mentioned that you could get some dies forged for me under your direction. I must own I wish you would for I want more than my Smith can do. I have therefore sent you one of my Dies by Coach and wish you would get me twelve dozen (!) forged exactly to the same height and diameter, they are about two and a half or two and five eight diameter and one and a half inches high in the cylindrical part and two and five eight from bottom to top. I beg you set a man instantly to forging and if he succeed I shall want a great number.
>
> These dies are steeled quite through and take from one and a half to one pound ten ounce of steel. They are for striking penny pieces of the size I have sent with the die but as they are struck in Collars they require a very hard blow and if not steeled through the dies would sink in the middle. If the lower part were common blister steel it might be tryed but I fear the wielding (welding) it to Cast Steel would injure the quality."

The observation of better performance if the impression was cut into the side of the bar is interesting. This would avoid possible problems due to segregation of impurities in the center of the forged bar.

The large number of dies ordered suggests that even with the best steel available die life was limited, either due to wear or breakage, and that many replacements were needed.

Imported steel from England and Sweden (regular blister steel and probably also the Huntsman type of crucible melted "cast" steel) will probably have been available in the port cities of America during the late 1700's. Boulton's letters certainly confirm the importance of steel quality for the severe service of coinage dies. Any die-maker who used steel of questionable quality would have been very likely to experience problems.

When we consider the many critical operations involved in making a coinage die we must have the greatest respect and admiration for those skilled craftsmen of two hundred years ago and for their remarkable achievements.

I wish to acknowledge much invaluable help in preparing this paper from Dr. Kenneth Barraclough and Mr. Charles Blick, both distinguished members of the British Historical Metallurgy Society. In particular, Dr. Barraclough supplied the transcript of the very relevant Boulton-Huntsman correspondence.

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Making Iron & Steel: The Historic Processes 1700-1900 (18 pages paperback)
by Jack Chard
Published by the author at 92 Windbeam Avenue, Ringwood, New Jersey 07456

Biographical Sketch of the Author

Jack Chard was born in England and graduated in metallurgy from the Royal School of Mines, University of London. He worked in the Firth-Brown Steel Research Laboratory in Sheffeild, England, and later in the Government Scientific Service in London during World War II. He was later Chief Metallurgist at the Royal Navy Torpedo Experimental Establishment in Scotland and in 1954 emigrated to a similar position at the Canadian Armament Research and Development Establisment at Quebec. In 1957, he joined the International Nickel Co. Research Laboratory in Bayonne, N.J., later relocated in Sterling Forest, N.Y. He has for many years been an active member of the North Jersey Highlands Historical Society and has served as its President.

Our special thanks to Gary A. Trudgen of Endwell, New York, who, after meeting Jack Chard at a local historical society meeting, and learning of his background and technical interests, invited him to submit this paper for publication in CNL. JCS

Money of the Colonies and Confederation: A Numismatic, Historic and Economic Correlation

by
Philip L. Mossman, MD
Hampden, Maine

●●● CONNECTICUT REVISITED ●●●

(TN-131)

Over three years have elapsed since my paper, "Money of the Colonies and Confederation: A Numismatic, Historic and Economic Correlation," appeared in The Colonial Newsletter as Serial #74 in September 1986. As patrons kindly submitted suggestions, corrections , and additional data, I was able to prepare an emendation of the original text which appeared in February 1988. At that time I announced my intent to edit the entire work for final publication. I am glad to report that this project, which proceeded more slowly than hoped, has just been completed.

In my original paper, I grouped the Connecticut series according to mint, style and date, following standard conventions. In the revision of Table XI, page 134, I added a great many more specimens. I prepared individual file cards for every undamaged copper from auction sales available to me where the weight was recorded. In this way, since each variety could be examined individually, weight differences between die types became immediately apparent and could be subjected to statistical analysis. Because of this closer scrutiny, I made some interesting observations which prompted me to enlarge the Connecticut series from ten groups in Table XI to fifteen groupings in the revision. I would like to share these new findings with the C.N.L. patrons for your interest, comment, and input. My revised Connecticut data are presented in the accompanying table.

Since the average weight for the each of the first three types from the Company for Coining Coppers (1785 MBR, 1785 MBL and 1786 MBL) fall within 2.7 grains of each other, these three types are combined and included here as group 1a. As a composite group of 182 specimens, their average is 136.3 grains. Considering the large size of this sample, there is a less than a 0.0001 probability that this group would fall within the 144 grain standard.

The 1786 DBL (group 1b) are too few in number to allow any positive statements. The average weight and standard deviation are both high reflecting heavy, yet inconsistent, planchets. More specimens must be measured before it can even be implied that these early Draped Bust varieties were minted under different circumstances than their fellows.

The 1787 DBL (large letters) and the 1787 MBL, both from the Company for Coining Coppers, could not be distinguished from each other by weight and so are combined as group 1c; these coppers exceed the legal requirement. I used Breen's criteria of letter size for separating the Draped Bust Left varieties according to mint.

In my earlier Table XI, I treated the 1787 DBL output of the Jarvis mint as a single entity. But when each variety is subjected to metrological analysis, there is a very interesting development which is otherwise hidden in the average of the larger group. Considering the large size of group 2a, this group does not compare favorably with the 144 grain standard, and as group 1a, does not meet the legal requirement. There are two subsets within the proposed Jarvis output which appear to be heavier than the others.

Revised Table XI

Description of Group*	Number of Specimens**	Weight in Grains	Compared to 144 Grain Standard***
1) Company for Coining Coppers			
1a) 1785 MBR, 1785/6 MBL	182	136.3 +/- 11.0	p=<0.0001
1b) 1786 DBL	9	154.7 +/- 16.1	exceeds
1c) 1787 DBL/MBL	59	144.8 +/- 11.2	exceeds
2) Jarvis Mint 1787 DBL			
2a) 1787 DBL	303	136.6 +/- 11.4	p=<0.0001
2b) 1787 "g" reverse DBL	18	143.2 +/- 13.6	p=0.7975
2c) 1787 "pheons"	43	149.1 +/- 12.3	exceeds
3) 1786 Atlee Dies	21	118.2 +/- 20.9	p=<0.0001
4) 1787 Mould Dies	32	123.5 +/- 10.0	p=<0.0001
5) 1787 Muttonheads	10	132.6 +/- 12.0	p=0.0145
6) 1787/8 Machin's Mills	57	115.1 +/- 14.2	p=<0.0001
7) Triple Leaves			
7a) 1787/8	78	122.3 +/- 17.2	p=<0.0001
7b) 1787 Miller 11-K	5	156.4 +/- 6.1	exceeds
7c) 1787 Miller 9-D, E, & R	9	137.4 +/- 26.5	p=<0.4779
8) 1788 DBL	48	120.5 +/- 14.3	p=<0.0001
9) 1788 MBR and MBL overstruck on CONSTELLATIO NOVA coppers	23	112.9 +/- 6.7	p=<0.0001

* = MBR = Mailed Bust Right
MBL = Mailed Bust Left
DBL = Draped Bust Left

** = data for 15 or less specimens included only for interest and at best may only indicate a trend and no conclusion should be drawn. More data are obviously needed in these cases.

*** = individual groups are statistically compared to the Connecticut standard of 144 grains. The observed weights are considered to be within range of the Connecticut standard of 144 grains when "p" is equal to, or greater than, 0.05. Whenever the "p" value, or probability, is less than 0.05, this means there exists a greater than 95% probability that the observed differences in weight between the standard of 144 grains and the average calculated weight of the coins are significant and not due to a chance occurrence.

The first of these exceptions is the population with the "g" reverses, including Miller 17-g.3, 18-g.1, 19-g.4, 22-g.2, 24-g.3 and 24-g.5. These varieties all have crosses on both the obverse and reverse. Unfortunately the sample size is too small to draw any firm conclusions, but when group 2b Is statistically compared with the larger group 2a, we can be 98% sure (p=0.02) that the "g" group differs from the remainder of its DBL colleagues of group 2a. Mike Hodder reported to me (personal communication) that he has a file of 52 specimens of "g" reverses with an average weight of 143.5 grains, values that are in excellent agreement with mine, although we are probably duplicating some of the same coppers in our two series.

There is a second and even heavier subset represented in group 2c consisting of Miller 34, 36, and 37 with "pheons" on the reverse (e, h, i, k, l, cc, dd, ee, ff, HH, RR). Many of these varieties also have an "R" substituted for a missing "B" in "LIB" in the reverse legend. When isolated and examined separately from the 1787 DBL, this combined group far exceed the authorized 144.0 grains and tip the scale at 149.1 +/- 12.3 grains which is a highly significant increase over group 2a. It is intriguing to think that the punctuation of Connecticut legends is a code and that the "pheons" and possibly the "crosses" signal some change in technology or sponsorship. Although there are insufficient "g" specimens to form any absolute conclusions, group 2c is sizeable enough so that the increased weight is statistically significant. This deviation from the Jarvis's other coppers would indicate that those varieties with "pheons" were minted under different circumstances. Further research is required here, but suffice it to say for now, the 1787 Jarvis DBL coppers are not as homogeneous as my earlier table stated.

There are no surprises in groups 3 to 6. The Machin's Mills output of 1787 and 1788 are only about three grains apart In average weight and hence are combined in group 6. It Is of interest that the Machin's Mills coppers on original planchets are virtually Identical In weight to their overstruck varieties separated out in group 9. The final DBL production In 1788 are very much lighter than any of their earlier counterparts.

Within the Triple Leaves varieties in group 7a, there is only a 3.5 grain difference between those dated 1787 and 1788. However, there is just a suggestion of two other heavier populations which have been split out for further examination as groups 7b and 7c. The Miller 11-K is very heavy on uniform planchets, but their numbers are too few to validate any speculation. Hodder has twelve 11-K In his records, with an average of 142.4 grains. While lighter than the five in my current table, they still exceed those of group 7a. Group 7c has immense variety in planchet weight as confirmed by the standard deviation on 26.5. Members of this group share very fine punctuation marks, and are intermediate in weight between groups 7a and 7b. Again, we need more specimens before we can make any claim that there is diversity in the Triple Leaves type.

In conclusion, what does all this mean; why the excitement over these weight variations? First of all, the original purpose for the state coinages was to provide a reliable, true, copper medium which for many years had been adulterated with light-weight counterfeits of reduced intrinsic worth. Thus the challenge of the Confederation mints was to correct this fraud which had been foisted upon the public for so many years. The various states established weight standards for coppers minted within their jurisdictions; for Connecticut it was 144 grains of pure copper. Any deviation below this standard would mean increased profit to the contract minter at public expense. Many contemporary legislative and newspaper reports specifically mentioned light weight coppers and the need to remedy the evil. The new Confederation copper coinages, themselves, deteriorated in quality over time and failed to bolster public confidence in this medium whose circulation came to an abrupt halt in the summer of 1789 for

a variety of reasons. Hence, this measuration of coin weights serves as an index of the minters' reliability and success in meeting standard requirements. From a numismatic viewpoint, the achievement of the weight requisite for a particular style of type of copper may also assist in identifying the mint of origin assuming that the quality of workmanship and fulfillment of the weight requirement is a reflection of a particular minter's standard of practice. Similarly, an alteration in weight may indicate different production methods in planchet preparation .

The conclusions derived from the current table are that ;

1) Half of the types produced by the Company for Coining Coppers exceeded the state's standard, whereas the 1785 issues (group 1a) did not. It is interesting to speculate why these early coppers failed to meet stipulated requirements.

2) The Jarvis 1787 DBL varieties are not a homogeneous group as far as weight Is concerned. This raises the speculation that those coppers marked with "pheons" were minted under some other more reputable sponsorship with a greater concern as to honest weight. The "g" reverses may also have been made under different circumstances than the other typical Jarvis coppers.

3) The few 1788 Miller 11-K stand out from their colleagues in regard to weigh and consistency of planchet size. We must have more specimens measured to either support or refute any suspicion claim of unlqueness, and, therefore, a different source than the other Triple Leaves varieties.

4) All other Connecticuts, with some possible exceptions, fall way below legal requirements and take their place among the deteriorated, light-weight coppers they were envisioned to replace.

5) What we obviously need from Patrons are the weights of more specimens from groups 1b, 2b, 7b, and 7c to help answer these questions.

THE MOST COMMON CONFEDERATION COPPER

In the preparation of the Table XI revision, generally from auction sources, some 1984 Confederation issues were counted, excluding Fugio coppers and those of English manufacture. The interesting question is raised, what is the most commonly encountered survivor today submltted to auction? Unless the consignment is from a famous collection, there is the distinct tendency to offer only those specimens popular with collectors such as specifics type coins, rarer and interesting issues, and those in higher grades. A run-of-the-mill 1787 Connecticut Draped Bust left copper would not attract the same attention as a Vermont "baby head" of similar grade . Since Table XI is more heavily weighed towards New Jersey material, due to be the nature of the auctions available to me, no absolute statement of frequency

within the entire Confederation series can be made. Despite these obvious sampling errors, I found it interesting to do a census of specific varieties in the population available for my study. The following is the enumeration of the five most common Confederation coppers encountered in the revision of Table XI:

Place	State	Year	Copper	Number	Comments
1st	New Jersey	1787	56-n	30	"camel head"
2nd	Connecticut	1787	4-L	18	"horned bust"
2nd	New Jersey	1786	17-b	18	50% overstruck
3rd	New Jersey	1787	64-t	16	
4th	New Jersey	1786	18-M	14	"bridle"
4th	New Jersey	1787	46-e	14	
5th	New Jersey	1788	67-V	13	three way tie
5th	New Jersey	1787	63-S	13	
5th	Connecticut	1788	2-D	13	

Within the Connecticut coppers, the first four place coppers are also listed. All of the five coins below are type coins and it is evident that the 1787 Draped Bust Left varieties are so common, that only notable specimens would attract auction interest.

Place	Year	Copper	Number	Comments
1st	1787	4-L	18	"horned bust"
2nd	1788	2-D	13	
3rd	1787	14-H	11	arrows in legend
4th	1787	13-D	10	"childish head"
4th	1787	1.2-C	10	"Muttonhead"

The most striking conclusion from these data is that the overstruck New Jersey camel head, Maris 56-n, represents by far the most common Confederation copper submitted to auction where weights have been recorded. Perhaps its popularity in auctions is because collectors are encouraged to add more than a single specimen to their cabinets due to the large variety of interesting host coins available. Considering the number of modern day survivors, this must have been a very substantial coinage. Is there any way we can estimate the original population?

There is debate as to whether the "camel heads" were from Machin's Mills or Elizabethtown. The arguments will not be repeated here except to state that the host coin utilization for the Maris 56 clone is far more characteristic of the Elizabethtown practices where all available lighter weight coppers were fed into the presses. Since the Maris 56-n dies lasted so long, it is also evident that whoever minted them had the capability to anneal the host coins since if they were not softened, the dies would have long since shattered.

comprises almost 4% of all the New Jersey coppers examined. This does not even address the fact that the prodigious Maris "n" reverse was even more durable than its obverse partners and accounts for over 5.5% of all the New Jersey samples in my revised Table XI. Although we have concluded that the number of "camel heads" consigned to auction is inordinately large and doesn't represent their original proportion within the total New Jersey coinage, it is still a hefty number. If we let our imaginations soar and consider that there were about four million New Jersey coppers of all varieties, then at a maximum of 4% of the total, the original census for 56-n would be about 160,000, while the "n" reverse would have endured over 225,000 impressions! There is no way we could ever derive original mintage figures, but we can state with certainty that it was the largest colonial coinage. Even half of this maximal estimate for 56-n would be 80,000, still an impressive figure. What I'm getting at is that there is no way that a single die pair could have been responsible for all this progeny.

Spilman suggested that 50,000 impressions was an excessive expectation for the Fugio die pairs.(1) Hodder in a recent article, calculated that in 1796, less than 20,000 large cents were minted per die pair.(2) Even postulating that the finest current technology was available to the authors of the "camel heads," then there must have been more than one die pair to account for this tremendous output. Such a theory would require the minter to have had several completely hubbed dies, including the legends, for 56-n. Since coin detail can be difficult to interpret because of overstriking (only two of the 30 specimens did not mention a host coin) the existence of several 56-n die pairs could be hard to prove by naked-eye examination. If this notion of several completely hubbed dies is plausible, then who did it? Could Atlee have achieved Buell's hubbing technology? Whatever time may reveal, we are left with the facts that the 56-n variety was produced by a highly skilled artisan. By the same token (excuse the pun), the clones of 56-n, namely 57 and 58-n, were far fewer in number; only one 57-n and twelve 58-n's are recorded in my series. Since the obverse devices on these varieties are so similar, could these have been partially hubbed obverse dies which fractured earlier than Maris 56 ? So we are left with the question, is the currently observed census of the Maris 56-n New Jersey copper due to an original prolific output, perhaps employing more than one die pair, or is it frequency more apparent than real because of a significant modern-day sampling error? This is the challenge for future research using photographic imposition techniques.

We are spared this argument of more than one die with the Connecticut Horned Bust, Miller 4-L, since one can easily follow the progressive obverse die break. The 4-L, in this study, comprised 2% of all Connecticuts, and the "L" reverse was 2.3%, having been combined also with Miller 1787 obverse 1.3. These percentages are without doubt inflated since the 1787 DBL census is underrepresented in the total volume of Connecticut coppers in the table, and the Horned Bust, in its various stages of damage, is very popular with numismatists.

NOTES

1. "Some Comments on the Fugio Cents of 1787", *C.N.L.* 24.

2. "The Life of a Die," *Rare Coin Review*, Autumn 1989, #74, 34-35.

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Letters - Letters - Letters -

On Stephen West
• • from Eric P. Newman **(TN-122A)**
St. Louis. MO

Regarding Stephen West's "Fractional Currency Notes" (CNL No.82, page 1091) ——— When the Stephen West script was printed, Maryland had several types of exchange: (1) to try to comply with Proclamation Money Laws, (2) actual Maryland Money of Account, and (3) the commercial trade practices of neighboring Pennsylvania. The latter was the basis for the text (7 shillings 6 pence) on West's notes. All outstanding Maryland paper currency was redeemed by 1765.

The error I made in the name of Stephen West resulted from a reference which I followed but was at that time unable to verify. William R. Mumford of Annapolis was kind enough to advise me in November 1983 of the correct detail and that is included in my third edition of *The Early Paper Money of America* (1990).

The query as to the apparent inconsistency of exchange values can also be commented upon. The problem in Maryland when paper money was reintroduced in 1767 was that most Maryland unofficial transactions were circulated in Pennsylvania Money of Account (7 shillings 6 pence Pennsylvania Money of Account to the Spanish Dollar). Maryland was still obligated to comply with the Proclamation Money laws which limited anyone paying or exchanging more than 6 shillings of Maryland Money of Account to the Spanish Dollar. Maryland in its 1766 Act determined to abandon Maryland shillings as a unit of value and changed to dollars based upon 4 shillings 6 pence sterling to the Spanish Dollar. This problem is set out in more detail in my article entitled "The Earliest Money Using the Dollar as a Unit of Value," *The Numismatist* (November 1985), p. 2181.

From the above, the strange difference in exchange value of a Spanish dollar can be recognized because the 1767 Maryland issue has a sterling shilling basis and the West notes have a Maryland or Pennsylvania Money of Account basis.

At least I hope I have been of some help in clarifying this curious subject.

A Snippit from a "Christmas Letter"
• • from Sanborn Partridge
Proctor, VT

Numismatics: ...serious interest began in 1972, when I peeked at everything. But I soon learned the field is too vast to "shotgun", absent a few dozen oil wells or such, so collecting the Vermont coppers of 1785-88 became my top interest. For practical purposes there are 39 die combinations known, plus a new, unique 40th noticed in the Smithsonian collection. I had one variety of which only 3 examples are known and had probably reached an impasse on the missing pair, so I concluded it was a good time to bestow custody of my better ones to the Vermont Historical Society (VHS) and the ANA (American Numismatic Association), making final gifts on the installment plan. The result is to get the VHS to a superior collection of 37 varieties and the ANA to a fine collection of 31 varieties. I'm not entirely abandoning the field, for I still have examples of 29 varieties!

• • from ye Editor — Thanks!

Our thanks for your patience to all who have telephoned or written during the past few months. As most of you know, Huntsville was struck by a devastating tornado on Wednesday November 15, 1989. Fortunately *CNL* and ye Editor survived with only minor damage to a couple of trees; however, just one block down the street the destruction was total!

It was only a few days before electric and telephone service was fully restored, but our daily routine was severely disrupted and we are still behind with correspondence. The debris in the neighborhood is finally gone except in one or two locations, at places it had been four to six feet deep and required seemingly endless lines of trucks to haul it away. Thanks again — your concern is sincerely appreciated.

JCS

A Lead on William Buell?
• • from Chris Faulkner **(RF-37B)**
Ottawa, Ontario, Canada

I am enclosing a copy of a story from the Toronto *Globe and Mail* for December 13, 1989 on the subject of the city of Brockville, Ontario. You will note the statement "The city was founded in 1784 by William Buell, a Loyalist from Connecticut. After the American Revolution, he moved to Quebec where he joined the British forces. He became an officer with the King's Rangers and in 1784, after his unit was disbanded, Buell received a land grant. (A plaque commemorating his arrival can be seen on the corner of Home Street and Water Street West.) Buell divided his property and donated plots for churches and a courthouse. The settlement became known as "Buell's Bay." In 1812, it was renamed Brockville, after the war hero and administrator of Upper Canada, Sir Issac Brock.

Could this be the "missing" William Buell? [See RF-37 on CNL pages 369 & 444. JCS]

A Paper Presented by F. George Markham; March 9th 1894.
• • from David Bruce **(TN-132)**
Portland, OR

I am enclosing for the CNL Research Library a typescript of a paper entitled "Colonial Coins Before, During and After the American Revolution" Paper read by F. George Markham before the Capt. John Couch Branch , Connecticut Society — Sons of the American Revolution. March 9th, 1894. —— Women of the Ruth Hart Chapter, Daughters of the American Revolution, present by invitation.

Editor's note: All of the material appears to be accurate and I suspect that Markham extracted most of his information from "Crosby" which had been published some 20 years earlier. Unfortunately Markham listed no references — on page 10 is the statement "It is a matter of record that (Abel) Buell became an itinerant vender of dies and visited the states before mentioned peddling his peculiar wares as they were needed." It would be nice to know the specific records he was referencing! Evidently Markham was a dedicated collector since he states that he "owns nearly 150 varieties" of Connecticut Coppers.

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Did New Jersey Coppers Officially Circulate In 1792?

• • **from Michael Hodder** **(RF-64)**
Wolfeboro, NH

In reading the *Proceedings of the 17th General Assembly*, state of New Jersey, the following references to the New Jersey copper coinage may prove interesting:

> **Saturday, November 24, 1792.**
>
> The Speaker laid before the House Letter from the Treasurer, desiring that the Legislature would direct that the Coppers in the Treasury should be paid out in the same Manner as they pass generally from Hand to Hand, which was read, and committed to Messrs Van-Cleve, Vredenburgh and Fithian.
>
> **Tuesday, November 27, 1792.**
>
> Mr. Van Cleve from the Committee, to whom the Treasurer's Letter of the 23rd instant was referred, reported the following Resolution
> Resolved
> That when the Treasurer Shall find it necessary to issue any of the Coppers that now are in the Treasury, that he be and he hereby is authorized to issue the same at the Rate of Twenty-four the shilling. By Order of the Committee
> Benjamin Van Cleve
> to which the house agreed.
>
> **Wednesday, November 28, 1792.**
>
> Mr. Mayhew from Council, informed the house that Council had agreed . . .

What were the coppers referred to by the New Jersey General Assembly? They could not have been federal issues, since the "experimental" Birch and small cents were not struck in large enough quantities. They were, therefore, coppers available at the time in circulation in large enough quantities for their release to be a matter of concern for the Assembly. While it is not so specifically stated, it is nevertheless tempting, and quite probable, to suggest that the coppers referred to were the seigniorage payments made by the New Jersey coiners to the state, paid into the treasury by the contractors.

This brings to mind the interesting diary reference published by Gary A. Trudgen in the July 15,1989 issue of *Penny-Wise.* In May, 1794 the French emigre Moureau de St. Mery reached a Hackensack River ferry crossing, where he observed the ferry woman refuse to take a New Jersey copper as payment for the crossing from a passenger. As related by St. Mery, ". . . she refused it obstinately and became furious, declaring with the most expressive words that she didn't give a hoot for the Assembly of New Jersey, whose members were no better than she and couldn't make her take their money." It would appear from St. Mery's diary entry that the resolution passed by the 17th General Assembly in November, 1792 was carried out, and that the coppers authorized to be released from the New Jersey treasury were indeed New Jersey coppers.

This reference is interesting, not only for such late circulation of New Jersey coppers, but more importantly, for the fact that their release was an official, state sponsored one, coming long after New Jersey's ratification of the Constitution, which made coinage for the Union a federal concern.

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